

MEMORANDUM

5 Rev. (7-55)

TO: A. J. Wohlstetter

DATE: 1-24-62

FROM: D. Ellsberg

MEMO NO.:

SUBJECT: AMMUNITION ON RAND VIEWS, FOR USE IN ARGUING WITH FROMM, BOULDING, RIESMAN, WIESNER, NEWMAN, AND REVIEWERS OF HERMAN KAHN

COPIES TO:

The following extracts are taken from a RAND lecture first given in 1955, revised and issued as a P in 1958 and revised for publication in a book on systems analysis in 1961. Despite these revisions, I have a feeling the author hasn't looked closely at it since 1955. As I interpret it, it is of historical interest (and would be of extreme current interest to the critics mentioned above, if it really seems desirable to put it in their hands now) as an indication of the way RAND found it expedient to talk to Air Force officers in 1955 on the following subjects (all italics added):

1. The Nature of Victory and the Appropriate Target System in Nuclear War

a. "There is a whole spectrum of victories which we might arrange in order of decreasing desirability: (1) Victory in style: for example, being sure that all our cities and those of our friends will survive; (2) the survival of at least all our cities; (3) a 50-50 chance for the survival of a significant number of our cities; (4) a technical win--two Americans surviving and only one Russian.

b. "We would want in such a war to have a high confidence (or at least a good chance) that we could destroy the enemy's economy and administrative centers. And on the other hand, we might also consider the advantages of having something left to reconstruct of the enemy's economy and population." (p. 142)

c. "The enemy also has some fond desires which are symmetrical to these. He would hesitate to go into a war if he could not be fairly sure that he could destroy enough of our urban areas so as to reduce our military potential seriously." (p. 157)

d. "These objectives are interdependent. Achieving one affects the satisfaction of another; sometimes favorably in that we kill two birds with one stone, or a plant as well as an administrative center with one bomb. Even if the interdependence is favorable for the fulfillment of the real problem, it may complicate the analysis... at other times the interdependence is unfavorable. For instance, in our birds and stone, for one reason or another it may be important for us to discriminate; thus, we may want to attack counterforce targets and not cities." (p. 143)

e. "Consider some hypothetical campaign comparisons between two strategic bombing systems, one based and operated overseas and the other based in the continental United States, and in this example, to be fueled

Fidelity Onion Skin

January 24, 1962

exclusively by air. Figure 6.2 depicts there (purely hypothetical) comparative effectiveness in a counterforce mission, given U. S. initiative. The overseas based system destroy three-fourths of the Soviet strategic force (SUSAC); the U. S. based system only one-fourth. How do these systems make out against Soviet cities? And (remembering this is a two-sided war) what, in the face of our attacks do the Soviets do to our cities? Figure 6.3 purports to show this. The overseas based system knocks out well over three-fourths of the Soviet city targets and, because of its extensive demolition of the Soviet long range Air Force, the remaining Soviet planes are unable to saturate our defenses or do more than a moderate amount of damage to our cities. [Figure 6.3 shows about 10% of U. S. cities destroyed.] This would be winning the atomic war in style. The U. S. based air refueled system does consider damage to his cities--get about half of them--and so could be at least a moderate deterrent, but it does not succeed in staving off an even more massive destruction of our own cities.

"Would the capability shown for the overseas based system be a deterrent? The answer to that might seem to be a resounding 'yes!'" But Figure 6.3, like Figure 6.2, assumed we had the first strike, and after all, what we are supposed to be deterring is his strike. What would happen to him if he did strike first, assuming in one case that we had the overseas operating base system referred to and in another case assuming the U. S. based air refueled system? Figure 6.4 shows this for our two fictitious systems. With the first strike the enemy destroys almost three-fourths of our cities, a full three-fourths of our overseas based SAC, and the undestroyed remainder of our overseas force manages only a very minor retaliation against Soviet cities. Here he wins the war in style. [Figure 6.4 actually shows three-quarters of the Soviet cities destroyed in this case, and about 10% of the American; apparently this is a misprint though it appears in all three versions. Reversing this, the Soviets lose 10% of their cities.] The ZI based system, on the other hand, which did not do so spectacularly well where we had the first strike, does not do much worse when the enemy takes the initiative: at least the enemy does not come off unscathed. [Figure 6.4 shows about 40% of Russian cities destroyed in this case. Incidentally, on all of these charts the scale of "percentage destroyed" goes up to 125, although none of the systems considered achieve this degree of "overkill".] Which of our two hypothetical systems then is the better deterrent? It seems fairly clear that in this example at any rate it is not the overseas operating base system." (pp. 143-146)

f. "In discussing the objectives which a military systems analysis might help to further, I listed a whole hierarchy of desires ranging from delectable to desperate minimum. I said nothing of the enemy's objectives and the objectives he might have to fulfilling our own desires... It is not easy to introduce the enemy into our calculations in a way that assigns him the degree of freedom, which he in reality has, to frustrate our simple desire to maximize the number of his cities destroyed." (p. 151)

g. "We saw earlier that systems which look just fine where we get the first strike [three-fourths of Soviet cities destroyed, 10% of U. S. cities] can look very bad indeed when we do not. Designing a system which does well in

both of these contingencies, then, is of prime importance. Such a system might, for example, save our cities in case we get the first strike, and at worst, where he strikes first, insure that his own cities will be devastated. Such a system, illustrated in Figure 6.6, is a reliable deterrent and would dominate the two systems illustrated earlier in Figure 6.3 and 6.4. We are not likely to stumble upon this sort of dominance. It is more frequently the work of design." [Figure 6.6, labeled "Dominance by Design (A Two Sided Atomic War)" shows over 80% of Russian cities destroyed in a U. S. first strike, in addition to 75% of SUSAC and 10% of U. S. cities; a Soviet first strike results in 75% of U. S. cities destroyed but 80% of Soviet cities (a "technical win" for the U.S.)]

2. The Desirability of Striking First

a. "We might -- I would argue sometimes that we must -- consider a still broader menu of Air Force objectives... Let me discuss some of these alternatives under three heads: (1) Provocation, (2) Deterrence, and (3) Capability of winning the war once started.

"First, provocation. It is clear that in the past there have been advantages in getting into at least some sorts of war, and while this is a delicate matter, it is also possible that in the future there might be some sorts of conflict which would be to our advantage. This depends on the nature of the damage done to us, both physically and in our relations to the rest of the world, as well as on the certainty with which we could achieve our objectives in the war. In any case, it is apparent that one might use the Air Force to help stimulate this hypothetically advantageous conflict. While this is clear, I think it is also clear to most of us that there are a great many advantages in not getting into World War III." (p. 141)

b. "In talking about how to conduct a campaign to destroy his force on the ground before it hits our force and our cities, the advantages of having the first strike are obvious. Without it we are locking the barn door after some, and possibly most of our forces are stolen. (It is difficult not to conclude that we just can't countenance his getting the first strike.)... Moreover, since he has some intrinsic political advantage over us for engineering a surprise attack, we must contemplate his getting the first blow as a strong possibility. This is one contingency we must plan for." (p. 153)

c. "Let me recur to another pair of contingencies mentioned earlier: the case in which we get the first strike in a war against the Soviets and the case in which this desirable order is reversed. There are many who think the unsatisfactory order more likely; there are some who are more optimistic." (p. 179)

THOUGHTS ON THE COUNCIL BOOK

AFTER READING DRAPER'S "BEYOND BERLIN"*

Albert Wohlstetter

Draper's article is an extremely savvy analysis of Soviet statements and policy and the Soviet-Chinese relationship. Without naming Fromm, Draper takes a well deserved smack at him. Page 385.

"There is one school of thought which creates a pattern by a process of elimination. It considers some Soviet statements, especially the more aggressive and expansionist, to be 'ritualistic' and, therefore, of no practical significance. It interprets 'peaceful coexistence' as if the Soviets had unconditionally decided to renounce the use of force. It attributes major importance to the theoretical dispute between the Russian and Chinese Communists on the relation of war and revolution, and construes it to mean that the Chinese stand for an aggressive, the Russians for a non-aggressive, Communist tendency."

Eric Fromm has taken exactly this manner of interpretation in his contributions to the Committee of Correspondence. Draper suggests on this subject:

"At best, then, 'peaceful coexistence' means that the Soviets intend to put an end to rival social orders by peaceful means if possible and to coexist with them only as long as necessary. The slogan is, if anything, a description of the present, not a prescription for the future."

And he adds:

"Altogether too much has been made of the so-called ideological difference between the Soviet and Chinese Communist leaderships If ideological differences were the only ones at stake, they could be patched up with relative ease, but it is the struggle for power within the Communist world -- for two 'vanguards' instead of one -- that has envenomed them."

"The Soviets are not necessarily conciliatory because they profess to believe in 'peaceful coexistence,' and the Chinese are not

*"Beyond Berlin: Is There a New 'Balance of Forces'?", Commentary, November, 1961.

"necessarily incorrigible because they scoff at it. Both are conciliatory and incorrigible as the opportunity offers and their interests dictate. The notion that the ideological dispute over 'peaceful coexistence' makes the Soviets 'peace-loving' and the Chinese 'war-like' is an oversimplification that verges on political innocence."

I would add to Draper's excellent statement a piece of historical confirmation. The phrase 'peaceful coexistence' had its first famous use in connection with the famous Chou en-Lai-Nehru statement in the early 1950's on the principles of peaceful coexistence. This occurred during a peaceful or conciliatory phase of the Chinese Communists.

Draper takes "The essential difference on war and peace between the Russian and Chinese Communists" to boil down to:

"different estimates of the practical possibilities. The Chinese, who do not have a nuclear bomb and have not been able to achieve their aims in Formosa, despite some rather forceful efforts, see no reason why they should spread illusions about the 'peaceful road to socialism' or expect Western capitulation. The Soviets, who possess nuclear bombs and have far more successfully pursued a short-of-war policy, can afford to be, or appear to be, more optimistic. The Soviet leaders are not less aggressive than the Chinese, but they have more reason to believe that they can achieve their ends not by war but by the threat of war, not by 'devastating blows' but by the threat of devastating blows, not by nuclear bombs but by the threat of nuclear bombs." (Page 387)

Earlier on that same page Draper outlines Khrushchev's essential strategy as an attempt to convince the neutral world and ourselves that:

"We have entered a 'new stage' of history in which the odds have turned against us. Why fight? Why resist?"

Khrushchev has been trying to convince us that it was his rocket threats which forced us to capitulate at Suez and Indochina and North Vietnam. The first purpose of the threats of the nuclear tests in the atmosphere in connection with the Berlin crisis is to induce this same sort of capitulation.

It is worth analyzing precisely what balance of power would offer factual justification for Khrushchev's policy. The Russians would need more than simply a second-strike capability. What they are suggesting invariably is that they will start wars, or support wars of liberation, but they will turn any war that we fight into a nuclear war. ^{all-out} In other words, their policy is a policy of massive retaliation and assumes that we don't have a second-strike capability. [?]

In the Council book one of the leading concepts in the book is the relationship between the internal-external incentives for going to war. The internal incentives are those which are meaningfully describable in terms of the United States and the Soviet Union and their mutual apprehensions about attacks on their mainland. This has been a preoccupation of most of the strategic analysis that has gone on. In the case of Tom Schelling's especially brilliant statements on the subject of central war, it is an almost exclusive preoccupation. My contention in the book is that this is very inadequate, and I have always tried at least to suggest the relationship to third areas and other motives for war. Draper's brief comments on Khrushchev and the new balance of forces, suggests how Khrushchev represents the relation between the United States, the Soviet Union on the one hand, and the third areas on the other. An analysis of what Khrushchev is claiming and an evaluation of these claims would be a very useful part of the book.

Hughes's statement in the round table on "Western Values and Total War" in the October Commentary suggests the relationship of unilateral disarmament and the advocates of dramatic acts of renunciation, on the one hand, and their attitude towards third areas, on the other hand. Their attitude

in brief is simply to give them up. They are either indefensible or indigestible. That is, indefensible by us or hopefully indigestible by the Russians. These poor devils in the third areas, too weak to be helped, but quite capable of taking care of themselves once the Russians or the Chinese are inside their boundaries.

7/18/61

IN DEFENSE OF THINKING ABOUT DEFENSE

By

Albert Wohlstetter

"Those who speak know nothing,
Those who know are silent."

These words, as I am told,
Were spoken by Lao Tzu.
If we are to believe that
Lao Tzu was himself one who knew,
How come he wrote a book
of five thousand words?

"
Po Chu-i - A.D. 772-846

Many of P. M. S. Blackett's books, pamphlets and articles on defense, including his recent "Critique of Some Contemporary Defense Thinking" in Encounter, have used a good deal more than five thousand words. As a Nobel Laureate in Physics and a pioneer during World War II in applying the quantitative methods of science to the study of military operations, his words command our attention and respect. They have had mine. I have been a most attentive reader of his writings, though frequently a puzzled one. For much of his writing after the war seems determined by his political passions rather than objective

7/18/61

-2-

empirical analysis. His Encounter Critique in its apology for intuition on critical issues on defense policy amounts to an attack not merely on "some contemporary defence thinking" but on any attempt to think about defense at all.

Mr. Blackett, who issues stern injunctions to some nameless culprits relying on "highly simplified models," reaches certainty himself with an absurdly over-simple model and essentially no empirical data. He justifies this on the erroneous ground that there is no body of relevant data, since there has been no large-scale nuclear war, and suggests merely that conclusions must be checked by "conventional military wisdom" and "against the conclusions reached in a more intuitive manner by attempting to envisage the situation as a whole." But the familiar trouble with such Visions of the Whole is that they are essentially Ineffable. Precisely because there has been no large-scale nuclear operations, traditional military wisdom has not benefited by experiencing any such "situation as a whole." (We hope that it never will.) What then happens when one military man's Vision of the Whole fails to match that of another military man? The history of interservice

7/18/61

-3-

differences, to say nothing of the violent fluctuations in service opinion or, for that matter, the fluctuations in Mr. Blackett's opinions, illustrates the pitfalls of this bald appeal to "traditional military judgment" in an era of revolutionary change in technology. Military judgments are indispensable for evaluating those component operations which bear analogy to traditional or current peacetime experience. However, though Mr. Blackett's new exaltations of "military wisdom" in the large may warm the hearts of some of his less thoughtful friends in the Royal United Service Institute, it will embarrass the more thoughtful ones. Certainly it will not impress those civilians and military men who have a major responsibility for continuing hard choices among strategic alternatives today.

In the past, Mr. Blackett at least avowed the importance of quantitative empirical work to evaluate defense policy.* His present Critique, however,

*"Though a statistical argument about a possible real war may be just as false as a qualitative one about an abstract war, this is no reason for neglecting to acquire all possible numerical facts about the past, before the attempt is made to predict the future. As was aptly said a century ago by Charles Babbage: 'Nor let it be feared that erroneous deductions may be made from such facts; the errors which arise from the absence of fact are far more numerous and more durable than those which result from unsound reasoning respecting true facts.'"

7/18/61

-4-

illustrates a disturbing and increasing anti-intellectual trend in recent discussions of problems of peace and war. For this reason as well as for the substantive importance of the issues involved, an answer to Mr. Blackett has a wider interest than a quarrel between Blackett and these "new" writers. These new boys make up a large and rather diverse group of writers on arms and arms control. He attacks not only the substance of what they have to say but also their method, and even their humanity. Mr. Blackett is worried, not only by the words these writers use, but by the way these words might be used by others. Perhaps because of this concern Mr. Blackett's vision of the words themselves is rather blurry. Since the bulk of his attentions are devoted to "The Delicate Balance of Terror," which I published in Foreign Affairs in January, 1959, I will concentrate on answering his comments on that article.

I will touch on the questions of (a) the degree of instability of the strategic balance and the meaning of "instability," (b) "accidental" and limited wars, (c) arms control and unilateral disarmament, (d) the humanity or inhumanity of sober empirical inquiry on arms and arms control.

MEMORANDUM

TO: Ed Quade
FROM: Albert Wohlstetter
SUBJECT: CHAPTER 3

DATE: 8/20/62

I think Chapter 3 is a good job. I've read it once very rapidly and will read it again. Meanwhile, I have a few corrections of a minor nature and some comments that occurred to me in reading the chapter that have a more general import which might suggest additions to your chapter.

MINOR COMMENTS

First, two minor comments: I think your reference on page 34 should not be to Life but to Harper's, ("RAND: Arsenal for Ideas," Joseph Kraft, July, 1960). Second, is that the word "cleared" at the end of the second paragraph on the same page is inaccurate. This may not be simply a quibble, as I'll explain.

The label "For Official Use Only" is not a classification and therefore its removal is not clearance in the security sense. It's a label indicating privacy, and applies to things like the Pentagon phone book and personnel files of government and non-government employees. In fact, it really doesn't add anything to the injunction in RAND Research Memoranda, or, indeed, in RAND books or any other publisher's books, that extensive quotation requires permission. My own "private" opinion is that this is a very transient label. Aside from a certain residual timidity, it simply expresses a desire to avoid any delivery of R-266 to the Hearst papers. Reasonable enough. Neither Harry nor Fred nor I have any intention of releasing it to the yellow press and so I didn't make any fuss about this, but I think it's in your interest to minimize reference to this ambiguous constraint. I suggest (1) eliminating line 9* "but it's not yet cleared for open publication." This leaves open the question as to how open it is (2) I would also check to see whether other documents "For Official Use Only" are always referred to with that proviso. (It was pointed out to me recently that Tom Milburn, about a year ago, referred to R-266 with full title, including the Secret classification, in an open publication of the publication of the Political Science Association.* His paper was probably cleared. In short, I am suggesting making it as unobtrusive as possible. At most, just leave the reference and the footnote.

*Second paragraph.

**Concept of Deterrence: Some Logical and Psychological Issues," The Journal of Social Issues, pps. 3-11, V. 17, No. 3, 1961.

GENERAL COMMENTS

Of my more general comments, two are related. One concerns the problem of credit for the change in Air Force policy. The other concerns the question of whether the preceding Air Force policy and plans were "absurd," obviously wrong. The two questions are clearly connected. It's the sneaking belief that the Air Force must have been stupid if a study of ours brought about a decided improvement that underlies some though not all of our shyness about claiming credit.

1) The "causation problem"

I am of course aware of the shyness which we are accustomed to accord any effect we may have had on policy. In connection with the Base Study, where there is hardly any room for doubt as to cause and effect, I myself held up the publication of R-266, the final report. I did it specifically in order to give the Air Force time to put some of the principal recommendations into effect and to give me a chance to soften certain of my own sentences in the report which showed too clearly our responsibility. For example, I made "programmed system" read "formerly programmed system," inserted sentences to the effect that policy had changed, to put it politely and uninformatively, "as the result of the growing stockpile of Soviet bombs." I even inserted a few face-saving but factually inaccurate references to the B-36 operations as something of a model for the recommended B-47 strategy. (They weren't. B-36s, as the Air Force knew, just couldn't be serviced overseas. Maintenance was the consideration, not vulnerability. The B-36 operation wasn't designed from the vulnerability standpoint at all.) Something of this we had already done in selling the recommendations to make them more palatable. We did more after the recommendations were adopted.

But the question of causation in the case of the Base Study is not really in doubt at all. No one else was proposing anything like it any place in the Air Force, and an Ad Hoc committee of the Air Staff worked over it for several months analyzing the changes it implied. They made a presentation orally and in writing to the Air Force Council about it after coordinating with the unified and specified commands and with the Air Staff. The Air Force Council or C/S decision was made in a meeting with RAND people present after discussing the RAND presentation and the Air Staff Ad Hoc committee's analysis of the RAND study.

Our shyness sometimes stems from an actual inability to extricate what we've done from what others have done. Sometimes when we've been clearly right and others wrong, it stems from a desire to save face for our client. In the case of the Base Study there is no problem of extrication. It's just a case of saving face. But this last problem I think can be eased in several ways. Perhaps most important is to make

August 20, 1962

plausible the fact, which I think is a fact, that an Air Force strategy could be so clearly bettered without any implication that the Air Force was or is stupid. Another related and graceful way for RAND to save Air Force face is to point out that RAND bomber studies had exhibited the same preoccupation as the Air Force, and therefore the same neglect on the problem of ground vulnerability.

2) Why the Air Force position wasn't absurd or the Base Study results obvious.

I know of course that the basic results of the Base Study weren't obvious to begin with. They weren't obvious to me, and even the questions that I was pursuing hardly seemed interesting to all but a few of the people I talked with at the Air Force and at RAND. For example, as far as the results on ground vulnerability were concerned, after Harry and I had convinced ourselves we found an awful lot of people frankly incredulous. Even after we had done some preliminary calculations, Harry and I were unwilling to hazard a guess when we did our first write-up, D-1114, December, 1951. By the end of the spring of 1952 I was pretty sure but had enough doubts to spend a year in making sure that the major points weren't optical illusions. "The Delicate Balance...", published six years after completion of the study came as a shock to a great many people with access to classified information. Finally, the crucial role and difficulty of reducing the vulnerability of strategic forces which was one of the central results of the Base Study can't be terribly obvious because here it is almost ten years later, and a lot of people still don't understand it, including some Nobel prize winners. I have had occasion to brood about this last problem recently and have thought a little about the history of strategic bombing. I think that this suggests why it all wasn't obvious, especially if you had been in the strategic bombing business.

The problem of strategic bombing as it had been understood was to penetrate defenses, to bomb accurately, and the like. Throughout World War II, in fact, this had been the problem. Strategic bombers operated under conditions under which ground attack is unimportant, but it had not solved all of the problems that were important. The Air Force's conscious concern flowed almost entirely from the sorts of questions generated by their World War II experience. Moreover, it was not that they ignored the technological changes; rather they were concerned with how changes like jet fighters, surface-to-air missiles, new bomb-nav systems and payloads affected the traditional problems that they had been concerned with: namely, penetrating defenses and target destruction. When the Air Force thought of our own bombers therefore, it was concerned about the enemy's fighters and local defenses. They were not much concerned about the enemy's bombing force.

August 20, 1962

To show how pervasive this concern was, one needs to look not only at the war plans but at R & D. The development planning objectives and the general operating requirements for bombers for years after the Base Study as well as before hardly dealt with the problem of surviving the enemy's offense. Not only the Hustler but the B-70 was developed with penetration problems uppermost. The nuclear powered bomber, up until near the very end of its history, shows the same focus very vividly. The bombers would have been concentrated at billion dollar maintenance bases with the aircraft in a condition requiring days to make ~~it~~^{them} combat ready.

As an illustration of the fact that this was not simply lack of intellect, I would cite three additional pieces of evidence:

(a) The Navy never destroyed SAC bombers on the ground. In 1949 the Navy made a root and branch attack on strategic bombers. They were very eager to show the uselessness of the B-36. They talked about lots of things: the immorality of bombing women and children, the terrible efficiency of modern jet fighters and of local defense missiles, etc., etc. However, though the B-36 operation was one of the most vulnerable, (for example, because it was one of the least adaptable to "alerts,") the Navy never mentioned it. The central problem of the vulnerability of strategic forces escaped them.

(b) The Russians took even longer than we to get a glimmer of an understanding of this problem. Their bases were even more poorly located, their operation less alert, etc., etc. Moreover, the English, for whose intellects I have a very high regard, never got the point until very late in the development of their Blue Streak program, even though the second man in their scientific establishment, who was a delegate at the Geneva Surprise Attack Conference had the benefit of a succession of lectures on the subject by both Harry Rowen and myself. The French and the Germans are pretty muddle-headed on it right now.

(c) The third piece of evidence I have already suggested. RAND had a great many bright and able people. It did many useful studies. Many involved innovation. Yet RAND's studies of strategic bombing dealt essentially with the questions that had been issues in the history of strategic bombing: How penetrate defenses and what targets to select. It dealt with these questions, of course, more systematically, subtly, and with close attention to the current and future states of the art. But it looked at speed, altitude, bomb load, small versus large planes, etc., essentially in their performance through enemy defenses and over targets. By and large RAND bombing systems analyses matched our bombers against his defenses; our defenses systems analyses, with minor exception, matched enemy bombers against our active defenses.

August 20, 1962

Point (c) suggests face-saving possibilities if mentioned in a RAND publication. The whole question has, I think, an interest and importance beyond the question of credit and face-saving. In fact, in my own chapter in your book I have several brief mentions of inertia which are related. I refer there to the importance of dealing with inert strategies on the part of an adversary, and to the fact that inertia on our side is to some extent inevitable, given the necessary size of the defense bureaucracy, etc., but it offers an opportunity for the inventive systems designer detached from the bureaucratic grind.

The force of inertia, I think, is hard to understand for a rationalist like Herman with whom I have discussed this problem a number of times. Yet even in less bureaucratized institutions like the exact sciences most inquiries pursue old questions and sometimes, looking back, progress in the discovery of the obvious seems amazingly slow. I once recommended to Herman that he read Boyer's "History of the Calculus" to see how fantastically slow going it was, how many hundreds of years it took, to eke out points in the limit definition of the derivative obvious to any freshman. I think I may have also quoted from memory a statement by Whitehead: "Everything important in the history of ideas had been called obvious by someone who hadn't discovered it." I hadn't seen the sentence for over 25 years, and it turns out what Whitehead said was related but somewhat subtler. "Everything important in the history of ideas was said by someone who didn't discover it." I think that's true too. But not necessary in this case.

I have some other general comments on which I have made a few notes, but perhaps I'll get this off to you without further delay.

AW:lc

March 1960

ALBERT WOHLSTETTER, Associate Director of Projects, The RAND Corporation

Since 1951 at RAND, Albert Wohlstetter has conducted a series of studies concerned principally with the problem of deterring general war and the vulnerability of retaliatory forces. These studies influenced, among other things, the evolution of U.S. policy on the use of overseas bases and the development of "fail-safe" procedures for operating strategic forces so as to minimize the chance of accidental war. In 1956 he became chairman of the Strategic Air Power Group of The RAND Corporation, which included projects concerned with the active and passive defense of the United States as well as strategic offense. He became Associate Director of Projects at RAND in January of 1959, and Special Assistant to the President of RAND in November of 1960.

He has been a consultant to the State Department and was a Scientific Advisor to the U.S. Delegation in Geneva during the talks with the Russians on the prevention of surprise attack in November and December of 1958. Currently he is a Fellow of the Council on Foreign Relations.

Albert Wohlstetter was trained as a mathematical logician and economist. He was educated in the public schools of New York and at Columbia University. He was awarded a variety of scholarships and fellowships, mostly at Columbia. At the start of World War II, he was a research associate at the National Bureau of Economic Research; during the war, a consultant to the War Production Board and an official of Atlas Aircraft Products; and after the war, Director of Program for the National Housing Agency and a consultant and official of several industrial firms.

January 1960

ALBERT WOHLSTETTER, The RAND Corporation

Current:

Since 1951 has conducted a series of studies concerned principally with the problem of deterring general war and the vulnerability of retaliatory forces. In 1956, became chairman of the Strategic Air Power Group, which included projects concerned with the active and passive defense of the United States as well as strategic offense. Became the Associate Director of Projects in January of 1959, and Special Assistant to the President of RAND in November of 1960.

Honors and Fellowships:

Second Year and Graduate Honors, Phi Beta Kappa
Columbia University Faculty Scholar, 2 years
Columbia Dean Scholar, 1 year
Columbia University Fellow, 1 year
Social Science Research Council Fellow
Carnegie Research Associate
Graduate work in Economics after completing course requirements for a Doctorate and the Doctoral Thesis in Mathematical Logic

Positions Held:

Instructor, Braircliff College
Consultant to the Planning Committee of the War Production Board
Quality Control Manager, Production Control Manager, and Factory Manager, successively, Atlas Aircraft Products Corporation
Director of Research, General Panel of New York (R&D Company)
Director of Program, National Housing Agency
Vice President and then President of General Panel of California
Consultant to Cyclohm Motors, Induction Motors, Anza Realty and other companies
Member of Board of Directors of several companies

Publications in ISIS, Journal of Symbolic Logic, Philosophy of Science, Foreign Affairs, etc.

Membership in various scholarly societies including Symbolic Logic Association, American Economic Association, Econometric Society.